AN APPROACH TO WINGED FLIGHT

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CONTENTS

CHAP. PAGE

I. Of the Wings of Insects and of Birds ... 1

II. Of the Muscles of Birds 3

III. Of the activity of the Anti-gravity Muscles 6

IV. Of a Mechanical Imitation of the Postural function of the pectoral muscles ... 9

V. Of the terms A and V 12

VI. Of M. Auger's achievement 13

VII. Of what remains to be done 15

VIII. Of the Catapult Torse 17

IX. Of Silk 19

X. Of Experiments with a Silk catapult torse 21

XI Of the mathematics of the Catapult torse 24

XII. Of the meaning of the above ascertained facts 30

XIII. Of the Tension of the silk torse 32

XIV. Of the manner of the Flight of Birds ... 35

XV. Of the position of a Man in a Winged Machine 37

XVI. Of the corresponding Human Experiment 39

CHAP.		PAGE
XVII.	Of Later Tension Tests of a Silk Torse ...	41
XVIII.	Of a Twin-Arc torse frame	44
XIX.	Of an Atterrissage Provision for such Machine	46
XX.	Of rising from the Water	50
	Note to Chapter IV.	51
	Dr. Crossley's Letter	52
	POST-SCRIPT	54

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John D. Batten.

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PHOTOGRAPHS I. *to* V.

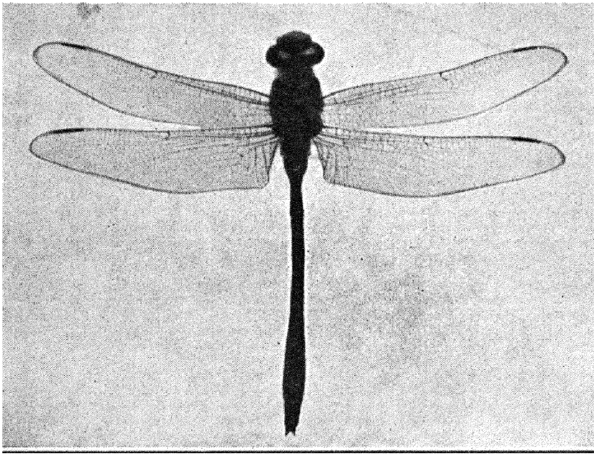

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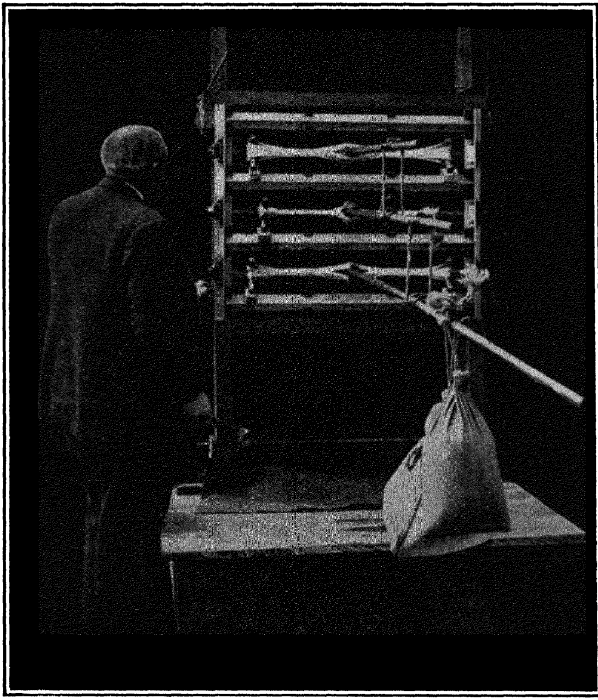
PHOTO I.

To CHAPTER I. THE ARCTIC TERN.

PHOTO II.

To CHAPTER I. THE ARCTIC TERN.

PHOTO III.



To CHAPTER X.

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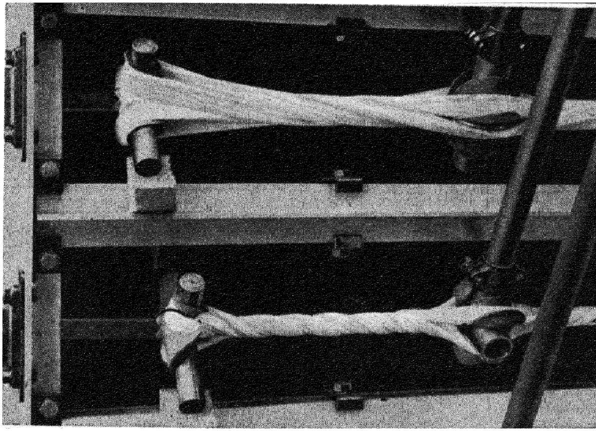

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PHOTO V.

CHAP. I. Of the Wings of Insects and of Birds.

WINGED flight, as we are able to observe it, is divisible into two main classes, the flight of insects and the flight of birds. There is also the flight of bats, but I am not sure that this differs fundamentally from that of birds.

Between the two main classes a fundamental difference exists, of the nature of which we have an indication in the shape of the wings.

The upper and under surfaces of the wings of insects are approximately alike, and the wings would therefore seem to be designed for an equal encounter with the air on either surface.

The upper and under surfaces of the wings of birds are dissimilar and the wings would therefore seem to be adapted to a different encounter with the air by the upper and under surfaces. The curvature of the wing would even suggest that the structure was intended to resist an air pressure always on the under side. That implies that there is an air pressure on the under

surface of the wing, not only during its downstroke but also during its apparent upstroke.

A consideration of the disposition of the muscles tends to confirm this suggestion.

The wing movement of insects (the butterflies and some moths excepted) is commonly so rapid that the eye cannot follow it. The wing movement of birds is observable by the eye, and in the flight of the larger birds the wing beats can easily be counted.

The flight of the humming-bird is exceptional. I have never seen a living humming-bird, but from all accounts it seems to be a bird with the flight of an insect. I know the flight of the so called "humming-bird moth."

On examining the stuffed specimens of these birds at the Natural History Museum, I find that the operative part of the wing is very nearly (like the wing of an insect) flat.

"The exception which proves the rule" is a phrase often used unintelligibly, but to the flight of the humming-bird it seems to be appropriate.

CHAP. II. Of the Muscles of Birds.

THE most conspicuous feature of the muscular structure of birds, and also of bats, is the enormous development of the greater pectoral. The evident function of this muscle is to depress the extended wing.

In the Natural History Museum at South Kensington is a series of exhibits arranged by the late Dr. W. G. Ridewood, illustrative of the skeletal and muscular mechanism by the evolution of which various creatures, Pterodactyls, Bats, Birds and Insects and, to some extent, Fishes have attained the power of flight. The evolution is obviously independent for each group.

In a "Guide" which Dr. Ridewood had written in explanation of these exhibits, the flight muscles of a bird are specially considered in the case of a wood-pigeon.

The great muscle is the *pectoralis major* which is strongly attached to the keel of the breast-bone (*sternum*) and also to the sides of the *sternum*. It pulls directly on to the first bone of the wing, the *humerus*, tending, in the attitude of flight, to depress the wing.

Underneath this muscle lies a smaller muscle, the *pectoralis secundus* also attached to the *sternum*, the upper part of which thins off into a tendon. This tendon passes through a small aperture between the bones, called the *foramen tri-osseum*, just like a cord through a pulley block, and its end is attached to the dorsal side of the *humerus*, that is to say, the upper side in the attitude of flight. This is the muscle principally concerned in the elevation of the extended wing.

In the pigeon which my brother dissected for the purpose of this illustration, the weight of the *pectoralis secundus* was found to be three-sixteenths that of the *pectoralis major*.

Reference: British Museum (Natural History) Guide (No. 6) to the Exhibition of Specimens, illustrating the modification of the Structure of animals in relation to Flight. 1923.

DISSECTION

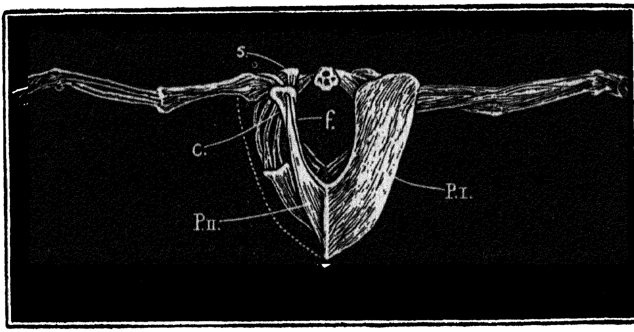

Fig. 1.

P.I. Pectoralis Major
P.II. Pectoralis Secundus
c. Coracoid
f. Furcula (merry thought)
s. Scapular

From the pigeon's left side no muscles have been removed, except two small muscles which stretch the skinfold (*patagium*) between the shoulder and the wrist. These are conveniently removed with the skin.

From the pigeon's right side all muscles are removed except the *pectoralis secundus.*

CHAP. III. Of the activity of the Anti-gravity Muscles.

It is now known that the muscles exhibit two kinds of activity—the one quick and transient and concerned with the execution of movement, the other relatively slow and prolonged and concerned with the maintenance of posture.

The former, which is termed 'phasic,' is a voluntary activity brought about by a rapid succession of impulses down the nerve fibre at the rate of about fifty per second. This kind of activity is accompanied by a great expenditure of chemical energy, by the production of heat and by the consumption of oxygen and output of carbonic acid. If unduly prolonged, or frequently repeated, this activity becomes rapidly fatigued and the muscle becomes incapable of further action until it has rested.

On the other hand, the postural activity is concerned not with the execution of movement but with the prevention of movement; that is, the posturally acting muscle develops within it a state of tension or resist-

ance to a force which is trying to stretch it. It is characteristic of postural activity that it is almost incapable of fatigue.

The postural activity of muscle is a proprioceptive reflex arising in the muscle itself, the adequate stimulus for which is stretching of the muscle. Thus, in man, the tendency of the lower limbs to bend under the body weight evokes a stretch reflex of the extensor muscles, which persists as long as the stretching force is acting. This is the mechanism by which the upright position of the body generally is maintained.

Such postural activity is conspicuously exemplified in the *antigravity muscles*, that is to say, in those that constantly resist and counteract the influence of gravity.

I suggest that in bird flight the greater pectoral is an *antigravity muscle*, and that during gliding movements its activity is postural and has in all probability that immunity from fatigue which is the characteristic of postural activity. This theory finds confirmation in the power of endurance which all of us have observed in the flight of the seagull and which many, more fortunate than I, have seen carried to its miraculous limit in the flight of the albatros.

In the address from which I have freely quoted throughout this Chapter, the distinguished Surgeon continues thus:—

Of the activity of the anti-gravity muscles.

"Students are taught that the action of the quad-
"riceps* is to extend the knee joint. It is nothing
"of the sort. It is true that the quadriceps does
"extend the knee, but that is a very small part of
"its action. The great function of the quadriceps is
"to prevent the knee from bending."

In like manner I would venture to say:— We have supposed that the action of the greater pectoral in bird flight is to strike the wing downwards. It does indeed strike the wing downwards, but that is a very small part of its action. Its great function is to prevent the wing from flapping upwards.

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*The quadriceps is the great muscle in front of the thigh, the lower end of which is attached to the knee-cap.

Reference: Proceedings of the Royal Society of Medicine, Section of Orthopædics. Presidential Address 'The Physiology of Muscular Action' by A. S. Blundell Bankart, M. Ch.—16th Oct., 1925.

CHAP. IV. Of a Mechanical Imitation of the Postural Function of the Pectoral Muscles.

It has seemed to me that in any human imitation of bird flight it ought to be possible to replace by springs or other resilient devices the postural activity of the bird's pectoral muscles, leaving to human energy the equivalent of the phasic activity of these muscles.

In order to estimate the relation which may subsist between these two orders of activity I have made the experiment indicated in the following diagram:—

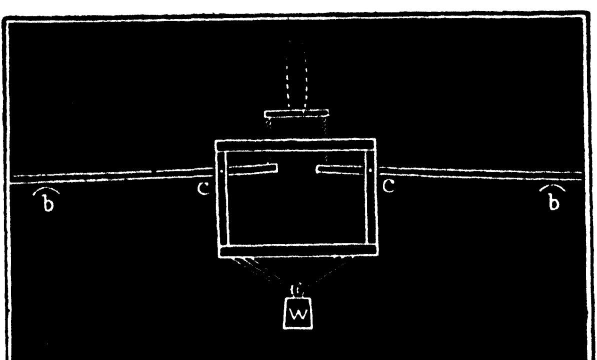

Fig. 2.

In the middle is a rectangular frame seen in front elevation. It stands for the thorax of the bird or the corresponding part of an aerial machine. On either side extend the wing spars. These are supported at b and b, the estimated position of the centres of air support under the supposed wings. At c and c are springs (the nature of which will be described later), tending to inflect the wing spars A-wards—meaning by A Λ as opposed to V. Weight is added to the thorax until the wing spars are depressed to the horizontal. To the inner ends of the wing spars, cords and a handle are attached as shown, and to the handle a loop of cotton represented by the dotted line. I find that with a single loop of No. 100 sewing cotton, which often breaks under a strain of 2 lbs. and which will never lift $2\frac{1}{2}$ lbs., I can dance the construction up and down for an indefinite time through a wing angle of 60° when it is weighted to 18 lbs.

The apparatus with which this experiment was made was of clumsy construction, and I do not intend to make the result a basis for any precise calculation until I have had an opportunity of confirming or correcting it by experiments with a properly constructed apparatus.

The first *full scale* experiment which I want to make is one in which a man occupying a position within the thorax and having control of the inner ends of the wing spars (which are supported as in the small experiment) is able to get the feel of the exertion necessary to

actuate a corresponding movement of the wing spars and thorax. I think the exertion will be found to be very small in relation to the total weight to be moved, that is to say, his own weight plus that of the machine.

See Note on page 51.

CHAP. V. Of the terms A *and* V.

I HAVE in the preceding chapter used the terms A and V to describe the angle of the wing spars in front aspect. I have done this because I wish to avoid, in this connection, the expressions "upstroke" and "downstroke," which I think would be misleading. A V shape can pass into an A shape in three different ways, as shown below.

It would be absurd to describe No. ii as a "downstroke," and it would be misleading so to describe No. iii which is the movement which actually occurs in the above experiment.

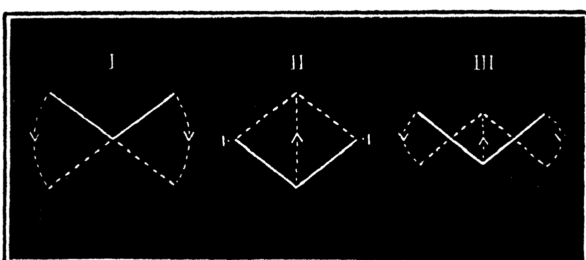

Fig. 3.

CHAP. VI. Of M. Auger's achievement.

It is reported in "Flight" that on July 26th, 1925 at Vauville, not far from Cherbourg, M. Auger on a motorless glider made an ascent of 720 metres, that is, 2,360 feet, and returned to his starting place. The land at no part of the Cherbourg peninsula rises, according to my map, much more than 300 feet above sea level, so it is unlikely that an ascent of 2,360 feet could be the result of any fortuitous upcurrent of the air.

I think we ought to accept as experimentally proven that a gliding ascent can be made upon a horizontal wind.

That is a thing difficult to believe.

I suppose the explanation to lie in the fact that no wind, light or strong, is ever constant. It consists of impulses alternately faster and slower. A movement alternately faster and slower is equivalent to a backward and forward movement within a constant stream. Given a backward and forward movement of the air, then if the intervals were regular and calculable, it ought not to be difficult to invent a pendulum parachute which would wag its way upwards. As however, the intervals are irregular and cannot be calculated, they have to be felt,

and this is what I suppose the gliding birds and extremely skilful aviators do.

In any case, M. Auger's achievement and that of every other glider may fairly be taken as a winged flight with locked wings.

If these wings had been unlocked, no doubt they would have flapped upwards, and the aviator would have come down; but if they could be unlocked and yet restrained by springs so strong that, until any other force were applied to them, they would retain their horizontal position, then it seems credible that their movement would be under control of the aviator, and that as soon as he knew how to exercise that control to his own advantage, he would actually be flying.

We are as near to it as that.

References:

(i) "*Flight*," *August* 13*th*, 1925.

(ii) "*Les Ailes*," *July* 30*th*, 1925—*Earlier in the day the wind is described as: "un vent assez irregulier de 7 a* 10 *metres a la seconde." Later the reporter writes: "Il supporte le grain violent qui s'abat sur le camp, puis ayant tenu l'air* 53 *mins.* 35 *secs. Auger revient atterrir aussi magnifiquement qu'il est parti."*

(iii) Aeronautical Research Committee, Report No. 732, *March*, 1921—*On the Soaring Flight of Birds, by L. le Page. This Report contains many very valuable sub-references.*

(iv) Aeronautical Research Committee, Report No. 969, *March*, 1925—*On the Katzmayr Effect.*

CHAP. VII. Of what remains to be done.

Two chief things remain to be done. First, the invention of a spring or other resilient device of strength and scope of movement adequate to the purpose indicated, but at the same time of no excessive weight, and, secondly, the devising of some method of preliminary practice and experiment which shall not involve too great a risk to life and limb.

Flying is, I take it, a human accomplishment to be learnt, like swimming, by practice, that is to say by failure and amendment. But which of us would have ever learnt to ride a bicycle (at any rate one of the high machines which I rode in my youth) if the condition had been to be killed the first time you came off? That, none the less, was almost the condition under which the pioneers of aviation made their attempts. Lilienthal indeed survived one mishap but the second was the end of him, else we should have been flying long since.

In later chapters I make suggestions on the problem of rising from and alighting upon the ground, but the

problem of the springs must come first, for until we have brought some simple form of wing movement within the control of the human will and muscles we cannot even begin to learn.

I think it is idle to attempt to determine the definite shape of the wing otherwise than by human experiment and failure. For us there is no lack of examples of possible wing forms. Let anyone think of the various contours of the wings of the eagle, the swallow, the gannet, the pigeon, the heron, and the owl—all noble flyers—and he will realize that our perplexity lies in the multitude and diversity of the examples set to us.

CHAP. VIII. Of the Catapult Torse.

For several years I have been making experiments with a view to solving the first of the problems named in the last chapter, that is to say, the discovery of a spring that should combine the requisite strength, range of movement and lightness.

Rubber immediately suggested itself but, rightly or wrongly, I have never felt confidence in the endurance of rubber and I have made no experiment in that direction.

I had steel springs made to a calculated strength and range of movement, but they were of excessive weight.

I then turned my attention to the kind of spring which I have described as a "*catapult torse.*" I mean the kind of device which the Romans used in their catapults and ballistae—a belt slung on a pair of carriers and twisted by a lever in the middle. It is evident from historic records that these catapults were of great power and played no insignificant part in military operations. As an engine of artillery—and I do not know that this

kind of torse was ever used for any other purpose—it has been completely superseded by explosives. I venture to suggest that the Romans never brought this machine to its full efficiency on account of their ignorance of the material best suited to the purpose—silk.

Reference: Vitruvius XV. to XVIII.

CHAP. IX. Of Silk.

UNTIL the time of Justinian, silk was known to the Romans only as a woven fabric imported from the East.

It is significant that the Romans, who must have been acquainted with ropes of flax and hemp, did not employ these for their catapults but made use of animal sinews or woman's hair. I suppose that a woman's hair was the longest thread of which the Romans had knowledge. It may be three or four feet long, but a thread of silk from a single cocoon may be anything up to three-quarters of a mile in length, and as it is the practice of the Chinese in reeling from the cocoons to twist together either six or ten threads and to connect a fresh cocoon as soon as the silk from one cocoon is exhausted, the finest thread of silk that comes under our observation is one of indefinite length, endless until it is purposely cut. This characteristic gives silk a great advantage over any thread spun from short lengths of fibre. When a thread of cotton

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Reference: "Silk" by Luther Hooper, Publ. Pitman.

breaks it is very seldom the snapping of the ultimate fibre (the "hair" as it is called by those engaged in this research); it is merely the pulling apart of "hairs" entangled by spinning. The longer the fibre the fewer the number of weak places in any given length of thread. Tables of comparative tensile strength based on the testing of two or three inches of fibre are therefore of no value for the purpose now in view.

The unsurpassed tensile strength of silk, though known to men of science such as those of the Silk Research Association, and put to actual use by surgeons in ligature operations, is not matter of common knowledge. Silk is commonly supposed to be a frail and perishable material. The reason is not far to seek; most modern silk fabrics, with the exception of those from Chinese or other oriental looms and with the exception of the output of at least one English house, are treated with acid. If you doubt, moisten the surface of any newly purchased silk and apply a strip of blue litmus paper.

During the bleaching process, it is not unusual to subject the silk to a treatment with dilute hydrochloric acid and, at a later stage, to a treatment with sulphurous acid.

CHAP. X. Of Experiments with a Silk Catapult Torse.

PHOTOGRAPHS IV. and V. show pretty clearly the construction of the frame used in the experiments. It is an upright rectangular frame of wood and duralumin, divided horizontally into three compartments, in each of which skeins of silk (ten in all) are slung, four, two and four, on cross-shaped carriers. Each group of skeins is twisted by a cylinder (or else a cross cylinder) in the middle, through which is thrust a long rod to form a lever upon which, when depressed to the horizontal, weights are hung.

These torses can be further tightened by the screw bolts and nuts of the carriers.

The levers can act separately or, if connected as parellelograms, can work together.

So connected, in an early experiment, the master lever supported at the horizontal a weight of 50 lbs five feet out, where it could be danced up and down without apparent fatigue to the silk. I believe that I could have tightened the torses to take a much greater weight but

I was doubtful of the strength of the frame. Further experiments have convinced me that the snapping of a skein of silk is not within sight; it is the frame that will go first. Anyone who grasps the lever will not only be convinced of the strength of the silk torse, but impressed by the resemblance of its rebound to the reflex of a living muscle.

The skeins used were of Chinese silk "*in the gum*," that is to say, in the state in which it is reeled from the cocoons. Examined under the microscope each individual thread is found to be a double thread (for the silk producing organ of the silkworm is double), enclosed within a single outer coating. The substance of this outer coating is soluble in boiling water though insoluble in cold. "De-gumming" by washing in boiling water is the first of the processes in preparing reeled silk for the loom. As the process of de-gumming is known to reduce the tensile strength of the silk, it seemed to me that for my purpose silk "*in the gum*" would be better, and I have not yet come across any disadvantage arising from the gum.

The silk is that described by Messrs. Grout & Co. of Yarmouth as "China Filature 18/20 den. Gold Double Eagle extra."

In reply to my enquiry, Dr. Denham, Director of the Silk Research Laboratory, Leeds, explains to me that the *denier* of a silk thread is the weight in grams of 9,000 metres and accepting as correct my weighting of each

skein as $3\frac{1}{4}$ ounces, states that the length of the (composite) thread of the skein, if continuous, will be about 48,000 yards. As I have ten such skeins in the three torses, I suppose that I have on the frame shown in the photograph, some 270 miles of silk weighing in all a little more than 2 lbs.

The torses have not been exposed to the rain but short of that they have been subjected to every change of heat and cold, damp and dry, that has occured within the last two years. Such changes do not seem to affect them in any way.

Reference: Advisory Committee for Aeronautics, Report No. 480, *August,* 1918—*Action of Water on Length of Threads, by A. Mallock, F.R.S.*

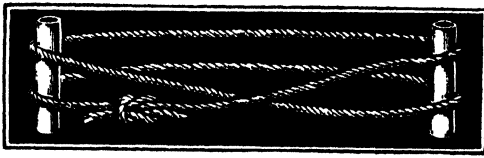

Fig. 4.

CHAP. XI. Of the Mathematics of the Catapult Torse.

In the experiments recorded in the last Chapter and in those which follow I have been in collaboration with my friend, Mr. Walter Stanley Bott, (M.I.Mech.E), without whose experience and assistance I should have found it difficult to build the necessary constructions or to carry through the experiments and record the observations.

My first torse experiments were not made with silk but with cords of flax or hemp. I noticed very early an apparently disproportionate increase in the strength of the torse due to a multiplication of the folds of the belt, and I determined to examine the phenomenon further by a series of breaking tests.

I could not make such tests with the silk above described because the threads are too fine to be handled or counted. I therefore employed Maclennan's Flax Kite Cord. It is necessary, of course, in such experiments to use a cord of uniform reliability, otherwise an increase in length merely increases the chance of coming upon a weak place.

The experiments all have relation to a continuous belt represented for this purpose by a cord passed a varying number of times round the carriers and tied in a single reef knot. Figure 4 shows what I describe as a *double* belt and as a belt of *four* strands.

The knot has not proved a point of weakness, but I think that by its pressure on an adjacent strand it has sometimes determined the position of the break.

The result of these tests is given in the annexed Charts.

It is evident that the strength of the torse, as judged by breaking strains, has a relation to the square of the number of strands involved.*

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*Though this relation has not, as far as I can ascertain, been investigated hitherto with this kind of torse, it is not altogether unanticipated as the letter from the late Dr. Arthur Crossley, F.R.S., which I append (page 52), shows. I have to confess that I find myself altogether out of my depth in the mathematics of the Cotton Research Association. The only fact which I am able to grasp is that a relation has been noted between W and n^2. For the benefit of those who have skill in mathematics, I give the appropriate references to the Memoirs of the above Association.

Reference: Shirley Institute Memoirs, 1923.
I. "The Rigidity of Cotton Hairs," by Pierce.
XXII. "The Plasticity of Cotton and other Materials," by Pierce.

The method of testing is as follows:

The cord is passed a given number of times round the carriers of the frame shown, sufficient slack being allowed to permit of the intended twisting.

A small cylinder is inserted in the middle of the belt so formed and is turned the required number of times, clockwise as seen from the left. A rod is thrust through the cylinder to form a lever, and the bolts of the carriers are tightened or loosened till the rod, carrying a minimum weight at the determined distance out, rests at the horizontal.

A further weight, added at the same point, depresses the lever. The bolts are screwed tighter till the lever rises to the horizontal.

This process is repeated till a cord snaps. The weight at which this happens is noted and, after correction for the weight of the lever, is recorded on the chart.

The rate of loading has a marked influence on the breaking load, and I think that the regularity of the readings of November 12th as compared with those of August 11th is due to the fact that Mr. Bott had at the later date acquired a uniform procedure in the conduct of the experiments. In any case, the trend of the curves does not appear doubtful.

For the November tests the over-all span of the torse was reduced to about 15 inches. It was found that 8 was the greatest number of strands that would take a torsion of 25 half turns. Beyond that the twists began to over-ride each other.

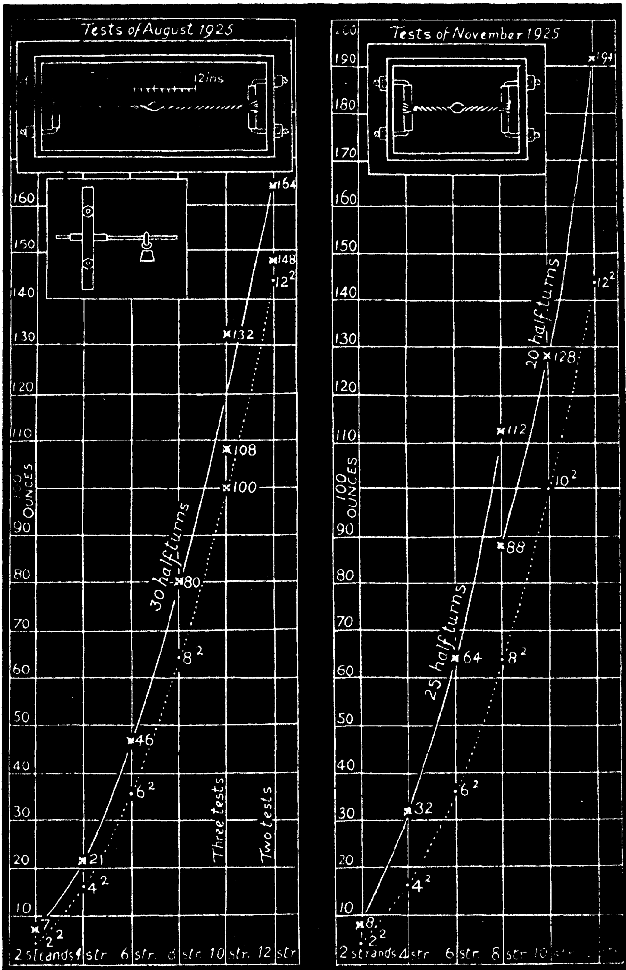

Tests of August 1925
12 ins
30 half turns
Three tests
Two tests
OUNCES
2 strands 4 str 6 str. 8 str. 10 str. 12 str
Tests of November 1925
25 half turns
20 half turns
OUNCES
2 strands 4 str 6 str. 8 str. 10

Fig. 5.

I therefore took a second 8 strand test at 20 half turns and continued the series of tests at that torsion.

As long as the catapult torse is regarded as part of an antiquated engine of artillery, no one will take the trouble to investigate its properties, but if it should be found to have a use in aviation, such tests as those above recorded become of importance. My hope is that they will be continued by those who have the apparatus and skill for making really accurate tests.

I submit that the catapult torse is a true machine, and that its behaviour is worthy of equivalent study with that of the pulley or the screw.

CHAP. XII. Of the meaning of the above ascertained facts.

I AM perplexed by the fact that the strength of the system, as judged by breaking strains, should increase as the square of the number of strands involved (and a bit more). This must mean that the strength of the system is advantaged in two different and distinct ways by a multiplication of the folds of the belt. Take for example a 3-fold belt. Seen in cross section the area of the strands is 3 times that of a single belt and it is not difficult to believe that it is 3 times as strong. That accounts for one way. Taken in total length the 3-fold belt is 3 times as long as the single belt (and a bit more, for more slack has to be allowed to accommodate the greater girth of the torse). That I suppose to be the second way. The greater length can, I think, only be to the advantage of the system by the elasticity of the cord. I imagine that with a wholly inelastic cord (if such a thing be) the advantage would be only 3 times and not 9 times. Beyond this I do not see clearly. But this I see, that if the advantage be as the square of the

number of strands involved, then in my silk torses in which the strands are in thousands, the multiple advantage must be in millions. Nothing has yet occurred in my experiments to throw doubt upon that conclusion.

CHAP. XIII. Of the Tension of the silk torse.

In strength, resilience, durability, range of movement and lightness, my silk torses give me all that I could desire. The drawback is the great strain put upon the torse frame.

In order to test the tension on the bolts of the carriers, I adopted a method recommended to me by the National Physical Laboratory at Teddington.

From a bar of mild steel 2in. diameter a number of $\frac{1}{4}$in. discs were cut and pierced with a $\frac{5}{8}$in. diameter hole in the centre to admit the bolt. Into one of these discs, three $\frac{3}{16}$in. diameter hardened steel balls spaced at 120° were deeply pressed at the above Laboratory.

In making the test a second disc is applied touching the three balls and the pair are passed over the screw end of one of the bolts and held in position by the nut which is screwed to give a normal tension to the torse. Upon the lever at the given distance from the hub a known weight is hung, and the nut of the opposite bolt is tightened until the lever at the horizontal supports the weight.

The weight is now removed and the nuts released. It will be found that three appreciable dints have been made in the second steel disc. A comparison of these indentations with those made by a testing machine of the National Physical Laboratory upon another disc from the same bar of steel affords a means of ascertaining the pressure on the disc, which is equivalent to the end pull on the silk torse.

The first test I made was on March 29th 1926. It was applied to the middle torse of the three [Photo IV] the one consisting of two skeins of silk, not four. This torse was first twisted on May 9th 1925, but near the end of that year I provided longer bolts and gave the silk six half turns, i.e. 1,080°. It is so shown in the photograph.

A weight of 30 lbs. hung 3 foot out on the lever at the horizontal, gave an end pull on the silk of 1·92 tons. The silk weighs 6½ ounces.

My misgiving is lest I should lose, in the weight of a frame of sufficient strength, the advantage that I have gained in the lightness of the silk. [But see later tests, Chap. XVII.]

The problem is definitely a mechanical one,—the association of the greatest possible strength with the least possible weight—and the like of it is being solved every day in other departments of aviation.

I defer to a later chapter my attempt to meet the dfficulty.

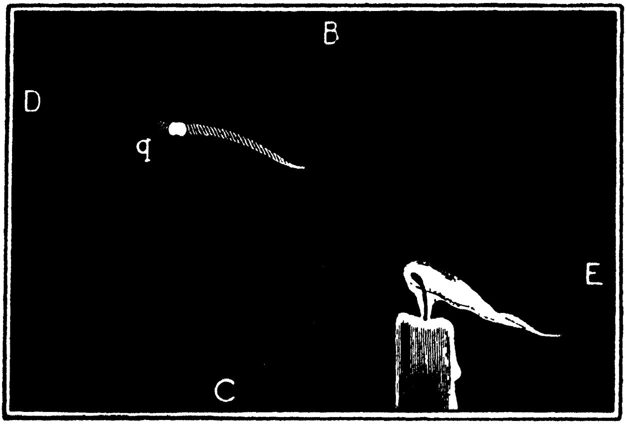


Fig. 6.

q. a quill feather in section.

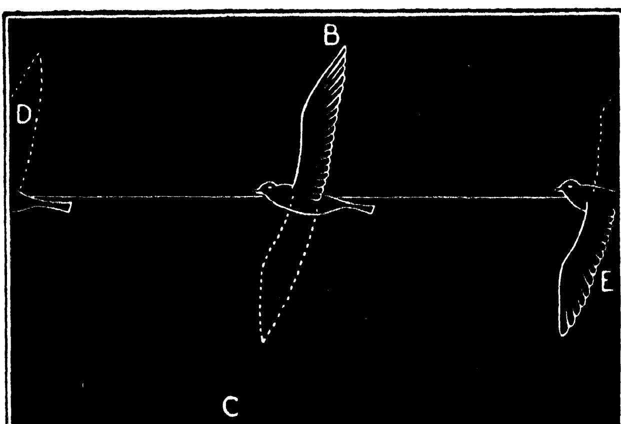


Fig. 7.

CHAP. XIV. Of the manner of the Flight of Birds.

ALTHOUGH the movements in the flight of any individual bird may be infinitely complex, the movement which is common to the flight of all birds is, I believe, a simple one.

It is not only the hawk, the swallow, and the seagull that can fly, even stupid birds and half-fledged birds can fly in a kind of a way.

The common movement I believe to be in this manner:—

Crudely stated, in the downstroke the wing behaves like a fan; in the uplift, like a boy's kite. If a fan, or other like thing, having a comparatively firm front edge and a yielding and elastic after edge (a piece of a quill feather of any large bird from which the front barbs have been cut away serves fairly well) be struck downward in the direction B—C (a not unusual direction for a wing-stroke), the greatest draught of air caused is in the direction D—E, which is equivalent to a propulsion of the fan in the direction E—D.

Translated into the wing movements of a bird, this means that a downstroke (in front aspect an inflection of the wings A-wards) propels the bird forward in a slightly ascending path.

The pectoral muscles are now partly relaxed (they are never wholly relaxed, unless the bird be shot, when it drops like a stone), and the thorax is allowed to fall between the wings, which continue to rise on the encountered air, just as a boy's kite does. During the first few strokes of flight when there is not sufficient encounter with the air, an obvious fall of the bird in the interval between the downstrokes is noticeable, but as soon as a sufficient forward velocity is attained, the bird by its skill equates the lift with the fall and thus maintains a horizontal course.

I submit that we go astray in our thinking if we fail to recognize that the apparent upstroke of the wings is an actual fall of the thorax—a fall from an ascending path. I am not denying that the fall may be accelerated by a twitch of the *pectoralis secundus* and I recognize that the wing points are necessarily thrown upwards by the inflection V-wards; none the less, the movement is essentially a fall brought about by a yielding of the greater pectoral muscles to the force of gravitation. I admit it looks like an upstroke, but then—How could it look otherwise?

This doctrine becomes of practical importance when we have to face the problems of rising from the ground and alighting on it.

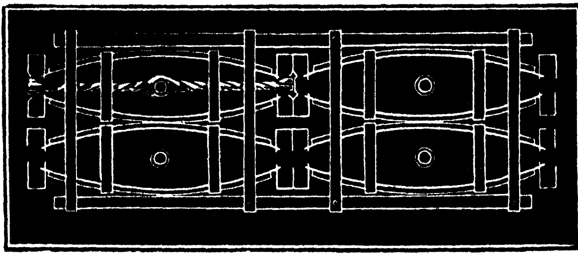


Fig. 8.

CHAP. XV. Of the position of a Man in a Winged Machine.

THE position of a man in a winged machine is determined by the desire to give him the fullest use of his strongest muscles in inflecting the wings A-wards.

Photographs VI. and VII. show the position chosen as represented in an inch to the foot model. This position allows a man the full thrust of his thighs and legs against the pull of his arms and back. By a miscalculation the rectangular frame (which by analogy I call the thorax) has been made too wide, and in consequence the man's hands are not as close to his sides as they should be for the full use of his strength.

I have assumed that 18 inches is the limit of a useful leg thrust, and as I have also assumed a wing movement through an angle of 60°, I am compelled to a length of 18 inches for the inner arm of the wing levers and of

the other commensurate levers. These assumptions are among the first which will have to be corrected by human experiment.

The shape and arrangement of the torse frames on each side is as shown:—

The upper torses actuate, that is to say tend to inflect A-wards, two main wing spars, fore and aft. These spars are connected to form a rectangular frame. Upon this frame it is intended that the experimental wing shall be built.

The lower torses actuate shorter spars, each auxiliarily to the spar immediately above it and attached thereto in such manner that throughout its movement it continues parallel to it.

For the model torses I have used ligature silk.

In the photograph it rather looks as if the man's hands grasped a bar in front of him. That is not so. Each hand grasps a bar at the side of him which forms part of a construction connecting the handle levers with the inner ends of the wing levers immediately above.

The bar in front is part of the saddle attachment. I do not intend to go further into details of construction, all of which are subject to amendment. I will only say now that the model machine and the model man do move concordantly, and that if the wing spars be supported at a convenient distance, the whole model can be put through the movements indicated in Chapter IV.

CHAP. XVI. Of the corresponding Human Experiment.

WITH reference to the concluding paragraph of Chap. IV., anticipating a full scale experiment:— The machine being similar to the model described in Chap. XV., the wing spar frames are supported at the estimated position of the centres of air support under the supposed wings. The torses are tuned to such tension that they will bring the wing spars horizontal when the weight of a man seated within the thorax is added. In such position of the machine the man's knees will be slightly bent.

The man by the thrust of his thigh and leg muscles and by the pull of his back and arms inflects the wing spars A-wards, and in so doing raises himself and the thorax of the machine and partly relaxes the tension of the torses.

He now relaxes the thrust of his legs and the pull of his arms; his weight and that of the thorax cause them to fall in relation to the points of support of the wing spars. The momentum of the fall carries them below the first position and the wing spars are in consequence inflected V-wards and the torses are strained.

Of the corresponding Human Experiment.

The man's knees are now more bent than at first and he is therefore able to give a longer thrust with his legs. His thrust is reinforced by the rebound of all the strained torses and he is able to inflect the wing spars more acutely A-wards.

From the higher position he falls with a greater momentum and so on.

If the analogy of the cotton thread experiment holds good, this performance will cost him very little effort and he will have plenty of strength to spare for the propulsive effort which will become possible when, in later experiments, wing forms are built on the spar frames.

CHAP. XVII. Of Later Tension Tests of a Silk Torse.

ON March 29th and 30th, 1927 I made the following tests with the four-skein torse, the bottom one in photograph VI. I had previously fitted longer bolts to allow for further twisting.

(i) With silk twisted to 405° at lever horizontal, a weight of 47 lbs., three feet out along the lever recorded, by the steel ball test, an end pull of 2·22 tons.

(ii) Twisted to 585° the weight at the same distance recorded an end pull of 1·32 tons.

(iii) At 765° an end pull of ·81 tons.

(iv) At 855° an end pull of ·54 tons.

(v) I twisted further to 945°, but I bungled the steel ball test and can give no reliable figure.

The overall span of the silk torse during these tests varied as follows: (i) 27in. (ii) 26½in. (iii) 25in. (iv) 24in. (v) 22½in.

These results give the curves plotted in the annexed Chart.

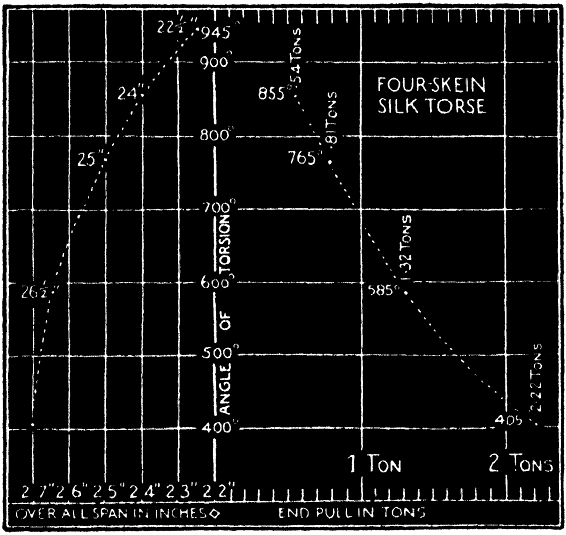


Fig. 9.

Later in the day, I tightened the nuts of the bolts sufficiently to give an overall span to the silk of 23in.

At this tension the lever supported at the horizontal a youth weighing 8 stone 2 lbs. hanging by his hands 2ft. 6in. out along the lever.

The diameter of the silk rope did not appreciably increase with the shortening of the over-all span. As near as I could measure it with callipers, it remained at 1⅛in.

The hollow spaces subtending the carriers and lever hub became, however, filled up with silk, some of it necessarily slack.

Throughout these tests I find myself experimentally in touch with a mathematics, the fundamental terms of which are obscure to me. The practical points ascertained are these:

(i) That in supporting a given weight, a given distance out along a lever, a 4-skein torse gives a much smaller end pull than a 2-skein torse. [See Chap. XIII].

(ii) That by increasing the twist of a torse from 405° to 855°, I can reduce an end pull of 2 tons to one of half-a-ton.

This makes the problem of a torse frame of sufficient strength much less formidable.

CHAP. XVIII. Of a Twin-Arc torse frame.

Photograph VIII. shows an attempt which I have recently made (Nov. 1927) to meet the difficulty stated at close of Chapter XIII.

My aim has been to diminish weight by translating thrust into tension wherever possible. The example which I have had at the back of my mind all the time, is the transition from a cart wheel to a bicycle wheel.

The frame consists of a pair of springs of silico-manganese steel, $\frac{3}{16}$in. thick, $1\frac{1}{2}$in. wide, chord 30in., shaped and tempered to the curve shown. The weight of the pair is 4 lbs. $3\frac{1}{2}$ ozs. The ends are held by blocks of duralumin and wood, through which pass the bolts of the carriers.

The springs are cushioned externally with wood, first, willow bent and outside that, beech, several pieces shaped. The springs are restrained from bending beyond the intended curve by vertical steel rods, each pair connected by horizontal pieces of duralumin. Diagonal wire bracing is added.

The total weight of the frame (not including the lever hub, the carriers, and the bolts and nuts of the carriers) is 7 lbs. 13 ozs.

The cross-shaped carriers, here set saltire-wise, are clamped to the main frame. I have avoided any other point of contact.

Photograph IX. shows the torsion of four skeins of silk, each weighing 3¼ ozs., supporting at the horizontal a steel lever weighing 16 lbs. upon which are hung weights amounting to 41 lbs., making a total leverage weight of 50 lbs. five feet out.

The skeins are the same as those shown at the top of the frame in Photograph IV. and have therefore been in torsion, on one or other of the frames, for 2½ years.

CHAP. XIX. Of an Atterrissage Provision for such Machine.

IN order that it may be possible to rise easily from the ground it is desirable that the wing movements appropriate to flight should be initiated before the machine leaves the ground. If the doctrine be true which is enunciated at the close of Chapter XIV, namely, that the apparent upstroke of the wings is an actual fall of the thorax in relation to the centres of air support under the wings, then it is necessary to allow room for the fall of the thorax even while the machine is on the ground. This precludes the use of an atterrissage part rigidly connected with the thorax.

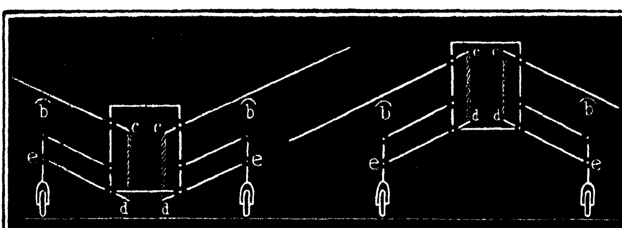


Fig. 10.

These considerations seem to dictate a construction somewhat on the lines of Fig. 10, in which b and b are the centres of air support and d—e is made equal to c—b. The inner lever-ends c and d are joined by a cord or leather thong of such length that when taut it brings c—d parallel to d—e.

The objection to this actual construction is that the radial movement of the atterrissage levers d—e would wrench the landing wheels sideways. I therefore considered whether I could place the atterrissage levers fore and aft instead of laterally, at the same time keeping the inner end d at the same distance from the inner end of wing lever c. This would obviate any sideways wrenching of the wheels.

A model which I made proved that this construction was practicable, the wing levers and the atterrissage levers moving concordantly. That is to say: when the atterrissage levers were horizontal the wing spars were horizontal; when the atterrissage levers were inflected upwards or downwards through any angle, the wing spars were inflected upwards or downwards through the same angle.

As the radial movement of the atterrissage lever is now in a different plane from that of the wing lever, the inner end of the former cannot continue immediately beneath the inner end of the latter, but if the leather thong connecting them be of fair length this does not cause any actual inconvenience. (At a later stage I re-

placed the aft atterrissage levers by a long skid. This saved some weight and did not render the construction impracticable).

With an atterrissage of these proportions the same limb movement of the man will inflect the wing spars to the same A-ward angle whether the support of the machine be at the appropriate position under the wing spars or whether it be under the atterrissage wheels. The man will therefore be able while on the ground to initiate the same movements which he requires when in the air, and on alighting he will be able to continue the same movement for a short time to the avoidence of shock. It was with the intention of making the man's limb movements identical when on the ground and when in the air that I have given the atterrissage levers a perhaps inconvenient length. If I modify that intention and allow a different limb movement in the different situations, the atterrissage levers can be shortened. In that case, if the torses are tuned to bring the wings horizontal when the machine is gliding, the wings will be inflected somewhat A-wards when the machine is on the ground and an effort of the man in an opposite sense to that of his usual effort will be needed to bring the wings horizontal. That is to say, he will have to pull his knees upwards against the down thrust of his arms. For this purpose a knee to foot stirrup attached to the thorax frame will be necessary.

While on the ground therefore the man would have to make with his legs an alternate push and pull. This will be a fatiguing movement but he will be relieved from it as soon as the machine leaves the ground. Even birds have to make exceptional movements when rising from the ground or from the water.

The earlier experiments, in any case, should be tried with the full-length atterrissage levers.

CHAP. XX. Of rising from the water.

In a machine intended to rise from and alight upon the water I anticipate that it will be possible to place floats in the position occupied by the wheels in Fig. 10. I think that floats can be invented which will not be rendered inefficient by the sideways wrench injurious to wheels. I hope that this may prove to be the case for experiments over water seem to me far less dangerous than experiments over land.

NOTE TO CHAP. IV.

I now see more clearly that this arrangement is a see-saw, established by balancing a system of springs (or of muscles acting merely as springs) against the total weight of the body, and that if it be allowed to oscillate at its own pace, the phasic activity necessary to keep it going indefinitely may be sufficiently stated as "next to nothing".

The cotton thread only measures the faultiness of the construction

Phasic activity will have to provide the whole of the propulsive effort, and propulsion will (in still air) have to provide the whole of the lift. [A bird flies by driving the air backwards, not by beating the air downwards.]

The function of the see-saw is to bring the wings intermittently into the position favourable for the propulsive stroke.

The huge development of a bird's pectoral muscles is, I believe, mainly for the purpose of establishing the spring balance, and if this can be established in any other way, a man will be at no disadvantage as compared with a bird in his muscular equipment.

THE BRITISH COTTON INDUSTRY RESEARCH ASSOCIATION

Shirley Institute,
Didsbury, Manchester.
29th December, 1925.

Dear Sir,

In reply to your letter of September 10th, which was originally sent to the College of Technology and finally found its way here, I have shown the contents to Mr. Pierce, the Head of our Testing Department, who comments on your letter as follows:—

The behaviour of this system is dominated by the elastic imperfection of the material. If the rope be kept at the same twist and approximately the same extension as obtained at the observation for a day or two, the conditions will be much more definite. (The elastic imperfection of textile materials is described in Memoir XXII., 1923)

The equilibrium state will then be approximately described in the equation.

$$\text{Torque} = Wl = nTaF + n^2T\Gamma$$

Where T is the twist, n the number of filaments each occupying a cross sectional area a, and having a static rigidity Γ and F is the tension on the rope. (The tor-

sional couple in a twisted bunch of filaments is analysed in Appendix A, Memoir I., 1923. The n^2 in the second term of r.h.s. applies when the threads are so tightly twisted together that relative slip is inhabited, for loosely twisted filaments it is to the first power.)

If the mean breaking load of the filaments be f, the equation at rupture is:—

$$W = n^2 \left(\frac{fa + \Gamma}{l}\right) T$$

It is rather surprising, considering the approximate character of this analysis, that a similar relation between W, and n, should be noticed empirically. The strength and manner of break of such specimens are subject to the effects of rate of loading and of the variability of the filaments, a discussion of which may be found in forthcoming Memoirs.

As the formula (2) is capable of quantitative test by measurement of the individual terms, I would be interested to see what degree of agreement is found. The breaking load and rigidity of the filaments should be measured under the same time conditions as obtained in the torque test.

I enclose you a copy of the paper which is referred to in Mr. Pierce's notes.

Yours faithfully,

The British Cotton Industry Research Association.

(*Signed*) Arthur W. Crossley,

Director.

POST-SCRIPT

THE chief obstacle which I have encountered is an opinion, based upon mathmematical calculations, that human flight is impossible.

I have been told: "The usual calculations made lead to the conclusion that man cannot fly by his own muscular energy."

But as birds undoubtedly fly by their own muscular energy, such an opinion needs the support of some argument based upon an irremovable difference between men and birds.

Man is certainly bigger than any flying bird, but as the condor is two thousand times the size of a gold-crest wren, and they both fly very well, it is difficult to believe that size can be a determining factor in the problem of flight.

It is also worthy of note that the large birds, the condor, and the albatros, fly with less apparent effort than the little birds.

It used to be believed that birds were of less specific gravity than men or beasts, because they had air in their bones. It is true that many, indeed most birds have air in their bones, but this provision appears to have no relation to their powers of flight, for, as is pointed out by Dr. Ridewood in the Guide to the Natural History Museum, from which I have already quoted, the fulmar,

the cormorant, and the petrel have no air in their bones, and the ostrich has.

In any case, the difference is so minute as to be inappreciable. The twig which a bird carries to build its nest must far exceed in weight all the potential marrow of its bones. Flesh and bone for flesh and bone, birds and men are of the same specific gravity, or if not the difference is negligible. The coat of feathers does not enter into computation. A man could put on a coat of feathers if he thought it would help him to fly, but he knows beforehand that it would not.

Even as between muscular and bony substance, the advantage is not always on the side of the bird. The adjutant, a bird supreme in flight, carries a huge bill. Even the toucan and the hornbill manage to fly somehow.

It is evident that winged flight does not rely for its accomplishment upon any extreme nicety in cutting down of weight. There is a margin, and a big one. We have recently been reminded that a golden eagle can carry a large Scotch hare.

It is in muscular equipment that birds have a conspicuous advantage over men. The whole of my invention is in the direction of supplementing man's deficiency in this respect. I do not pretend that the adoption of such invention would immediately enable man to fly. He would yet have to learn how. But I fully believe it would enable him to begin to learn.

Post-Script.

I am told however that it is very unsound to argue from the motions of birds and insects to the motions of large animals, and I am warned that a man observing the jump of a flea, might persuade himself that he could jump on to the top of the Eiffel Tower.

And yet, physiologically, birds and men are comparable at every point—bone for bone and muscle for muscle, brain for brain, and nerve for nerve. You can make no like comparison between a flea and a man. Why this distrust of a method of comparison which has proved so fruitful in other branches of research?

Do the Physicist and the Physiologist *never* meet?

I have at times felt compunction in thus inciting others to the adventure of a flight, the danger of which I am perhaps too old to share. I quiet my conscience with the thought that the danger does not include a risk of being burnt to death.

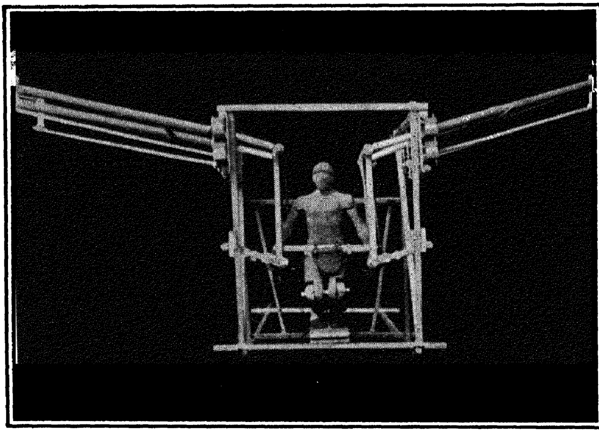
It would, moreover, be no small boon to humanity that flight should be silent.

John D. Batten.

Kew Gardens, Surrey.
December, 1927.

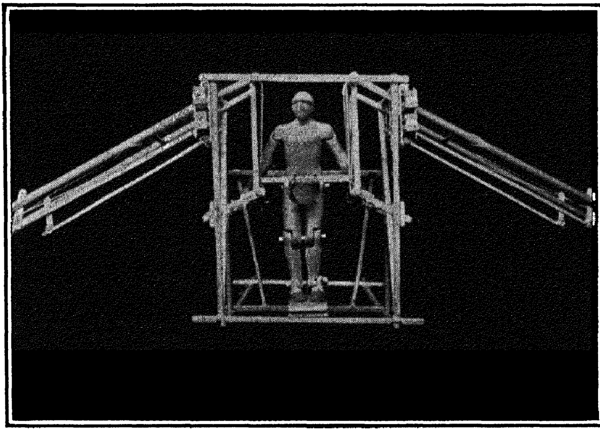
Reference: Journal für Ornithologie, Vol. LXX., 1922, *paper by O. Heimroth. Weight of Condor* 11 *kilograms, Pelican* 10 *kilograms, Swan* 9 *kilograms, Albatros* 7½ *kilograms. Weight of Gold Crest Wren* 5 *grams.*

PHOTOGRAPHS VI. *to* XI.



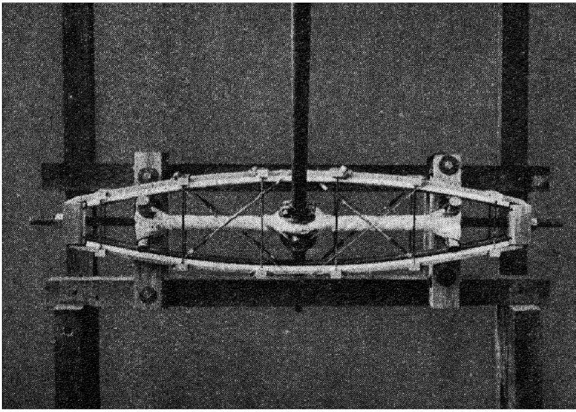
To CHAPTER XV.

PHOTO VI.



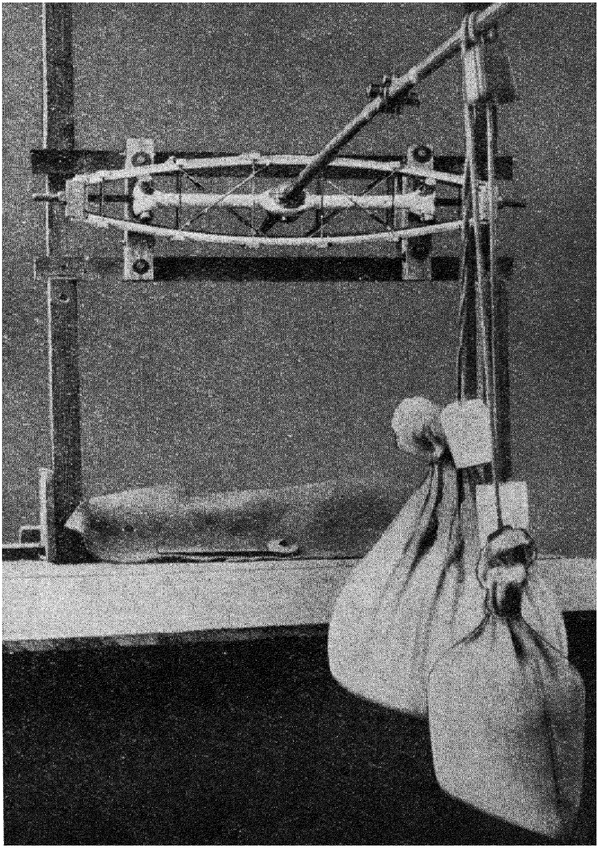
To CHAPTER XV.

PHOTO VII.



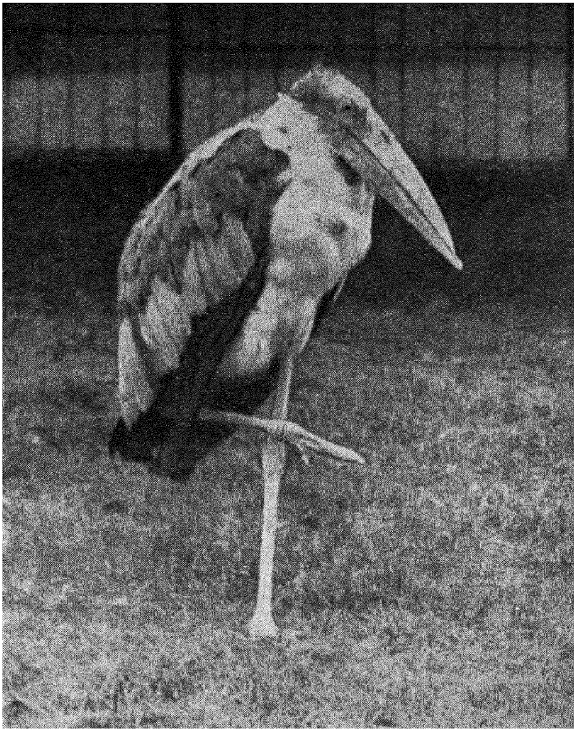
To CHAPTER XVIII.

PHOTO VIII.



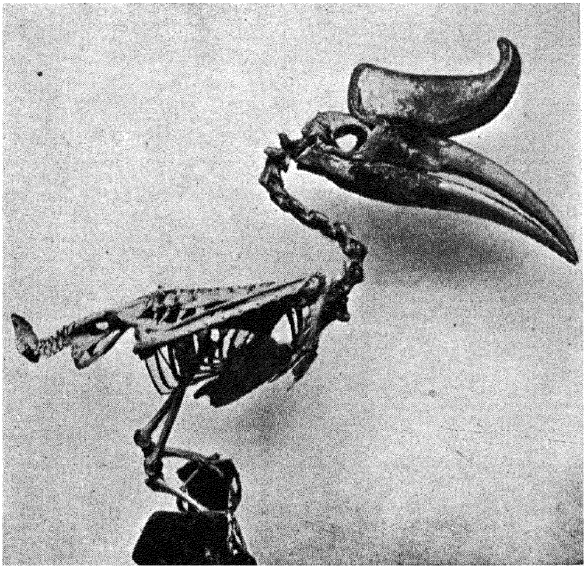
To CHAPTER XVIII.

PHOTO IX.



THE ADJUTANT.

PHOTO X.



SKELETON OF RHINOCEROS HORNBILL FROM THE BRITISH NATURAL HISTORY MUSEUM

PHOTO XI.

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PAPERS OF THE SOCIETY OF MURAL DECORATORS AND PAINTERS IN TEMPERA

SECOND VOLUME, 1907-1924. EDITED BY JOHN D. BATTEN. PRINTED FOR THE SOCIETY BY THE DOLPHIN PRESS BRIGHTON. PRICE 10/6

Extracts from Reviews

NATURE. August 1st, 1925 Arthur P. Laurie, D.Sc., Professor of Chemistry to the Royal Academy, writes:—

THE Society of Mural Decorators and Painters in Tempera is to be congratulated on having produced a volume of fascinating interest to those who are intrigued by the practical problems of the painter. : :

The painter of pictures and of wall decorations of to-day is in the unfortunate position of having lost invaluable studio traditions and having to rely on obscure and doubtful records of old methods of painting. The Tempera Society is, therefore, on the right lines in trying to bring together the experience of the painter, the information to be obtained from ancient records and the critical examination of old pictures, and in addition, the knowledge of the man of science.

The papers of Mr. Noel Heaton on the frescoes at Knossos and by the late Sir George Beilby on lime putty are of special interest. Mr. Tudor Hart is also to be congratulated on his excellent recipes for preparing egg and size emulsions.

At the close of a review of considerable length, Professor Laurie, writes—

In the meantime, all praise to the Tempera Society for its courageous attack on these problems. My only criticism is that they are not availing themselves sufficiently of the services of the chemist. The excellent work done, as revealed in this publication, by Burton, Noel Heaton, and Beilby, shows how much more we could do in the way of advice, criticism, and caution, and we are all anxious and willing to help.

JOURNAL OF THE OIL AND COLOUR CHEMISTS ASSOC. October 1925. H. Houlston Morgan, Ph.D. writes:—

WELL printed, attractively bound, and with an intriguing title, this volume not only provides pleasant and interesting reading for restful and recreative moments, but merits the serious and thoughtful attention of all interested in the painter's craft.

The progress likely to ensue by the co-operation of the artist with the man of science is very well illustrated by the two papers on 'Lime Putty' and 'The setting of Lime' by John D. Batten and the late Sir George Beilby. As both point out in the present volume, we are far from understanding the mechanism involved in the setting of Lime plaster. : : The term 'carbonisation' no more explains the setting of lime than does 'oxidation' explain the setting of a linseed oil film.

The two papers on 'The Mural Paintings of Knossos' by Theodore Fyfe and by Noel Heaton also show the value of such co-operation. The great revolution of opinion which resulted from Mr. Heaton's careful and well-planned investigation must have been a source of pleasure to him, comparable only to that felt by the discoverer of a new element or the transmutor of quicksilver into gold. The Society of Painters in Tempera may also take legitimate pride in initiating the investigation.

There are several papers describing, in a delightful way, the technique of the fresco painter and of the worker in tempera; there is also a short but interesting letter from Holman Hunt on the oil medium. The oil and colour chemist can quite profitably spend much time in studying these methods whereby the different workers in fresco, in tempera, and in oil obtain their colour and tone values.

The Society, before which the papers were presented, would appear to be doing good work, and is to be congratulated alike upon its efforts and upon the production of the present volume.

THE CAMBRIDGE REVIEW. February 12th, 1926. E. S. Prior, A.R.A., Cambridge Slade Professor, writes:—

IN the last year two books under the names of two practical painters have appeared, indicative, one may hope, of returning health to the art of painting: the one is Harold Speed's 'Oil Painting' and the other 'The Tempera Society's Papers, 1907—1924' (to give it a short title), edited, as will as contributed to, by John D. Batten. Those who are interested in modern painting should read both: for the point is that as well as analysing ancient art, these treatises treat of the painters' craft as it works—and not merely with painters' art as it is talked about. We have been passing through a sort of picture influenza this last thirty years—an inflamation of tne critical functions—caught abroad, but with symptons as virulent here, as elsewhere in Europe. : : But the critical test of the painter is still his action with the paint.

Mr. Batten appears here as editor of various papers contributed to the Society of Mural Decorators in view of establishing how the technique of of wall-painting may be revived. From the true fresco of the Minoan Knossos we go on to examine wall-grounds and painting processes, limes, gildings, varnishing, and pigments—taking their uses in medieval and Renaissance science down to the work of Mrs. Sargant Florence, and of Mr. Batten himself.

The attempts that were shown as mural decoration at Burlington House some years back indicated how unfitted the easel painter had become for either figure-work or landscape painted directly upon a wall.

THE OXFORD MAGAZINE. Feb. 25th, 1926.

Arthur M. Hind, Oxford Slade Professor, writes:—

THE present collection of papers is to a large extent of a technical character of chief interest to practising painters, but there are several articles which will appeal to the archæologist and historian notably those of Mrs. Herringham on the Ajanta Caves, of Mr. Theodore Fyfe and Mr. Noel Heaton on the Mural Paintings of Knossos, and of Mr. E. W. Tristram and Mr. J. D. Crace on English Medieval mural painting and medieval painting in general. Of the technical studies Miss Lanchester's examination of Le Begue's water wax recipe is one of the most significant on account of her conclusion that it was the method practised by the Van Eycks. A letter of Holman Hunt's, and a report of a meeting at his house, in which his own methods and those of Millais and Maddox Brown were discussed, will be of particular interest in relation to the Pre-Raphaelite paintings in the Ashmolean. Recent work is represented in the frescoes of Mrs. Sargant Florence at Oakham School, subjects finely conceived and carried out in a breath of style entirely consonant with the process. They are among the few recent examples of pure fresco in England, and it is of great value to have Mrs. Sargant Florence's description of the method used. Amateurs would be well advised to aquire one of the few copies of the Society's publication available to non members.

THE STUDIO. March, 1926.

AN admirable book dealing principally with the preparation and permanence of colours relating to mural painting, and in a lesser degree, to other forms of painting, both ancient and modern. The editor is to be congratulated on the impartial way in which he has selected the 'papers' of various experts of established reputation, who clearly state the exact results of their own artistic and scientific research work. For instance, Mr. Noel Heaton's paper describing his chemical examination of the mural paintings of Knossos (discovered by Sir Arthur Evans in the Isle of Crete) forms a sharp contrast, bearing in mind the antiquity of the palace of Knossos, to the paper by Mrs. Sargant Florence, who states her own technical experience of the Oakham frescoes exectued by herself in recent years. Other papers of real interest are contributed by John D. Batten,

William Burton, M.A., F.C.S., P. Tudor Hart, M. Lanchester and Lady Herrington, in addition to the excellent material supplied by other writers. The book deserves to be well known and is much needed in England where artists who are interested in mural painting very rarely have the opportunity of displaying their decorative instinct.

BURLINGTON MAGAZINE. March, 1926.

THIS society was founded in 1901 by Lady Herringham and a number of painters interested in technical processes, particularly those affecting mural painting. A great deal of investigation and many experiments have been made, the results having occasionally been published. The present volume contains some very interesting papers. Mrs. Sargant Florence's technical account of her experience in mural painting is of distinct value, as are the results of Mr. Noel Heaton's chemical examination of the series of mural paintings discovered in Crete by Sir Arthur Evans, and Sir George Beilby's consideration, based on microscopic examination, of the exact behaviour of plaster during the process of setting. There are in all twenty-one contributions to the book. They vary in value but none is without merit. It is almost unnecessary to say that all seriously interested in this important subject should obtain a copy of the publication and should get into touch with the Society.

The same Society, before it had assumed its longer name, issued a volume entitled

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